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Canadian Journal of Psychology

THE INHERITANCE OF BEHAVIOUR: BEHAVIOURAL DIFFERENCES IN FIFTEEN MOUSE STRAINS[1]

WILLIAM R. THOMPSON[2]

McGill University

As HALL has pointed out in a recent review of the literature (12), a science of psychogenetics is as yet more of a promise than an actuality, and such work as has been done in it has been fragmentary and often based on inadequate methodology. Consequently, it is highly desirable that there be initiated a programme of research which takes cognizance of the failings of previous studies and starts off on a firm empirical foundation.

Two main techniques have been used in approaching the problem. One has been the method of selection. By interbreeding only the extremes in each generation, a number of investigators have produced strains of rats which are widely separated in "maze-brightness" (13, 16, 25, 26), emotionality (10), and activity (18). The fact that such strains can be bred indicates that these traits definitely depend upon heredity. But the manner in which they are genetically transmitted is still unknown. Thus Tryon (26), for example, made crosses between his "bright" and "dull" animals, but since the variability of his F1 cross was as great as that of his F2, he was unable to undertake a genetic analysis. Similarly, Brody (2) made several crosses (F1, F2, and backcrosses) in the twenty-second and twenty-third generations of Rundquist's active and inactive strains. Although she concluded that the character of "inactivity" is transmitted by a single "inhibitor" gene, dominant in males and recessive in females, her results do not justify such a simplified interpretation, as Hall has remarked (12). Hall, in an unpublished study (12), crossed his emotional and non-emotional rats, but obtained considerable heterogeneity in the F1. Kuppasawny (16) has theorized that general mental ability is determined not by a single factor or by multiple factors, but rather by the entire set of parental genes handed down in a given combination. He has

[1]This research was supported by grants-in-aid from the National Research Council of Canada, the Rockefeller Foundation, and the Office of Naval Research, U.S.A., contract 1001–01, to the Roscoe B. Jackson Memorial Laboratory.

[2]The experimentation was carried out at the R. B. Jackson Laboratory, Hamilton Station, Bar Harbor, Maine. Thanks are due to Dr. J. L. Fuller, Dr. J. P. Scott, and other members of the staff for their valuable advice and assistance.

no incisive evidence from his experimental work to support such a hypothesis.

In general, then, selection studies have contributed very little to our understanding of how behaviour is inherited. This has been owing to two main difficulties. First, close inbreeding has not been carried on sufficiently long to produce genetically pure strains. As a result, any cross made is bound to be so variable as to preclude the possibility of further analysis. Secondly, the trait for which selection is made is often highly complex, and may represent the interaction of a hierarchy of related characters. Such was the case with Tryon's strains, as shown by Searle (22). This being so, it would seem essential to deal as far as possible with unitary traits, and to eliminate between the strains being bred any differences other than the one being specifically studied (25).

A second approach to the genetics of behaviour has been by the use of stock already known to be genetically homogeneous. By starting with pure strains one of the difficulties stated above is immediately eliminated. Yerkes (28), Coburn (3), Stone (24), Dawson (4), and Lindzey (17) have shown systematic differences in emotionality between various mouse and rat strains, but they did not pursue the matter far enough to gain any knowledge of the genetic basis of this trait. The studies of Bagg (1), Sadovnokova-Koltzova (19), and Vicari (27), dealing with maze-learning in different mouse and rat strains, showed considerable promise and deserved to be followed up. Unfortunately, they were not. From a genetic standpoint, the most profitable studies have been those initiated by Hall (11), and continued by Fuller and others (6, 7, 8), on the inheritance of audiogenic seizures in the mouse. These serve as a case in point, indicating the level of sophistication that can be attained in the field of psychogenetics. From a psychological standpoint, however, this work is of less importance. What is obviously needed is a thorough investigation of the normal psychological characteristics of some of the eighty-odd available mouse strains, followed by appropriate crosses and a genetic analysis.

The present experiment is the first step in such a programme. Specifically its purpose is the examination of food-drive, emotionality, and exploratory activity in fifteen mouse strains. These three parameters were chosen for two reasons: first, they are fundamental to many more complex types of behaviour; and secondly, they are measured with reasonable simplicity and with a minimum of handling of the animals.

The Experiment

Animals

Samples of 20 animals (10 males, 10 females) from the following 14 inbred mouse strains were used in the experiment: C57BL/6, C57BL/10,

C57BR/a, DBA/1, DBA/2, AKR, AK/e, A/C1, BALB/c, ND, C3H, TC3H, LP, BDP. These are described fully elsewhere (23). The hybrid strain "Obese" was also used. All animals had been reared under ostensibly identical conditions in the Jackson Laboratory colony. They were between 70 and 90 days of age at the time of testing.

Test 1. Food-drive and Emotionality

Apparatus. A modified Hall open-field test was used in this part of the experiment. It consisted simply of a standard mouse shipping-box (approximately 12 by 24 in.) with wire mesh top. In the centre of the box, on a sheet of paper, was placed a small glass dish containing 10 gm. of dry Purina mash mixed with milk powder.

Procedure. Ten boxes were used, thus allowing ten animals to be tested simultaneously. Each animal was put on a reduced ration of food, determined as follows: the mean weight of the animals was calculated, this being approximately 20 gm. Each animal was given daily one gm. of Purina checkers per 20 gm. of body weight during the six days of the test. Twenty-three hours after the mice had first been put on this ration, they were taken to the experimental room, and each put in one of the testing-boxes. They were allowed to eat and explore for ten minutes. At the end of this period, each was removed and returned to its living-cage in which it was given its daily ration of food. This procedure was repeated for the next five days. The amount of food consumed by an animal during each 10-minute test was determined by weighing the food in the dish before and after test. Since the amount of food that can be consumed by a mouse in ten minutes is not very large, a scale accurate to one-hundredth of a gram was used.

Defecation was used as a measure of emotionality, the scale being based on the frequency with which animals in a particular strain defecated over the six days.

Results. First, the total amounts of food eaten by each mouse of each strain over the six days of testing were compared. A rough plot made of these totals showed that they were heavily skewed positively. Consequently, for purposes of computation, they were normalized. An analysis of variance was then calculated on the transformed scores, with strains and sex as variables.[3] High significance was obtained between strains ($F = 9.32$, $p < .001$) but none between sexes. The interaction was not significant. Using the error (*within*) variance of this analysis, t tests were then made between each strain and every other one. The results of these

[3]It should be mentioned that in both experiments the variances within strains were unequal, as shown by L tests for homogeneity (Experiment 1, $L = .83$, $p < .01$; Experiment 2, $L = .84$, $p < .01$). However, since the Fs were large in both cases, it is unlikely that the conclusions drawn from the analyses are seriously affected.

tests are presented in Table I. It is clear that wide differences exist between the different strains in strength of food-drive. Out of 105 possible comparisons, 60 were significantly different. However, only the highest and lowest strains showed a complete absence of overlap in their scores.

TABLE I

MEAN GRAMS OF FOOD EATEN BY FIFTEEN MOUSE STRAINS OVER SIX DAYS, AND THE p VALUES OF DIFFERENCES BETWEEN THEM

No.	Strain	Mean gm. eaten	p values of differences*	
			.05–.01	.01–.001
1	TC3H	204	> 6	7–15
2	AKR	199	>	7–15
3	C3H	183	> 7	8–15
4	DBA/1	176	> 7–9	10–15
5	ND	171	> 7–10	11–15
6	C57BL/6	162	>10–11	12–15
7	AK/e	131	>13–15	
8	C57BR/a	128	>13–15	
9	LP	124	>13–15	
10	DBA/2	121	>13–15	
11	C57BL/10	116		
12	BALB/c	99		
13	Ob	82		
14	BDP	82		
15	A/C1	80		

*Read, for example, as follows: No. 1, Strain TC3H has a mean of 204 gm., significantly greater than Strains No. 6 (p = .05–.01) and Nos. 7 to 15 (p = .01–.001).

The second point of interest was the change in strength of food-drive from the first to the last day. The amounts eaten by each strain were plotted as a function of time and then compared. On inspection, there were found to be marked differences between strains in this respect. Since the irregularity of the curves did not permit an exact analysis of their slopes, the gain from the first three to the last three days was computed for each strain to allow a gross comparison to be made. These figures are presented in Table II. By comparing them with the scores in Table I, it can be seen that they bear little relation to total amounts eaten over all six days together.

Thirdly, a comparison was made between the fifteen strains with respect to emotionality as measured by frequency of defecation over the six days of testing. The percentages of animals of each strain defecating

TABLE II

GAINS IN EATING AMOUNT FROM FIRST TO SECOND HALF OF TEST 1
(Sessions 4–6/Sessions 1–3)

Strain	Gain in gm.	Strain	Gain in gm.	Strain	Gain in gm.
BALB/C	21.40	TC3H	2.05	AKR	1.66
C57/BRa	3.41	AC1	2.01	BDP	1.59
LP	3.18	C57/10	1.97	Ob	1.52
AK/e	2.44	C57/6	1.96	C3H	1.26
ND	2.20	DBA2	1.86	DBA1	.95

during the six days are presented in Table III. A chi square computed on these data was found to be significant ($p < .001$).

As might be expected, food-drive and emotionality were inversely related. The fifteen strains were put in rank order for each of these two variables, and a rank-order correlation run. The coefficient obtained was -0.796 which is significant ($p < .001$).

TABLE III

PERCENTAGE OF MICE IN EACH OF FIFTEEN STRAINS
DEFECATING DURING SIX 10-MINUTE TESTS

Strain	%	Strain	%	Strain	%
AK/e	96	A/C1	78	C3H	52
BDP	85	DBA/2	70	C57BL/6	40
Ob	83	C57BR/a	66	AKR	34
LP	82	C57BL/10	58	ND	27
BALB/c	82	DBA/2	57	TC3H	11

Test 2. Exploratory Activity

Apparatus. A square enclosure 30 by 30 by 3¾ in. with a wire mesh top was used in this test. The floor was painted gray, the walls and wire top a flat black. The floor was divided by pencil lines into 36 small squares, each 5 by 5 in. At the base of every other square was placed a single unit barrier of corresponding length and height, there being 15 of these in all. At one corner of and leading into the enclosure was a small starting box in which each animal was placed at the start of a test. A diagram of the floor-plan is shown in Figure 1.

Procedure. Each mouse was placed in the starting compartment and given 10 minutes to explore the enclosure. A record was taken of the number of lines traversed by each mouse, this being used as an index of

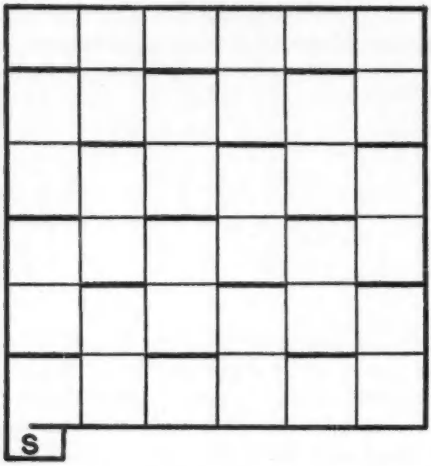

FIGURE 1. Groundplan of the apparatus used in measuring exploratory activity. Thick lines represent barriers.

strength of exploratory drive. For purposes of determining the reliability of the test, ten mice (five males, five females) from each strain were run twice, on consecutive days.

Results. An analysis of variance was computed on the data, giving the following results: the variance of strain means was significant ($F = 11.71$, $p < .001$). No sex difference appeared, and the interaction was not significant. Again, using the error term of this analysis, t tests were made between each strain and every other strain. The results are presented in Table IV. Of the 105 possible comparisons, 68 were found to be significant. There was no overlap between the highest and the two lowest strains, nor between the second two highest and the lowest.

The reliability of the test for the whole group of mice which were run twice ($N = 150$) was found to be 0.925, ($p < .001$). Within each strain, it varied somewhat, being above 0.900 in five cases, above 0.800 in six cases, and below 0.600 and not significant in four cases.

The Relationship Between Test 1 and Test 2

It seems plausible that there would be some relationship, either negative or positive, between the amount of food eaten in Test 1 and the strength of exploratory drive in Test 2. For example, one might argue that timidity or lack of it in an animal would interfere or facilitate performance in both tests. On the other hand, a strong tendency to explore might well be expected to draw an animal's attention away from food,

TABLE IV

MEAN AMOUNT OF EXPLORATORY ACTIVITY SHOWN BY EACH OF FIFTEEN MOUSE STRAINS, AND THE p VALUES OF DIFFERENCES BETWEEN THEM

No.	Strain	Mean score	p value of differences*	
			.05–01	.01–.001
1	C57BR/a	459	> 3–4	5–15
2	C57BL/6	361	> 7–9	10–15
3	C57BL/10	359	> 7–9	10–15
4	DBA/1	334	> 8–10	11–15
5	ND	308	> 8–11	12–15
6	BDP	286	>10–12	13–15
7	DBA/2	253	>11–13	14–15
8	LP	194	>13–15	
9	AKR	188	>13–15	
10	C3H	177	>13–15	
11	Ob.	149	>15	
12	TC3H	117		
13	BALB/c	74		
14	AK/e	60		
15	A/C1	20		

*Read as in Table I.

and so produce a negative relationship between the two tests. In fact, neither of these possibilities is supported by the data. A correlation between the two tests gave a rank-order coefficient of 0.150 which is not significant. Consequently, they may be considered to be independent.

DISCUSSION

To summarize the results, significant differences in food-drive, emotionality, and exploratory activity were found between a number of the fifteen mouse strains tested. Since all strains were reared in ostensibly identical conditions, these differences may be considered to have a genetic basis.

Contrary to the observations of Keeler (14, 15), no definite relationship appeared between the behavioural traits studied and coat-colour or other morphological characteristics. On a gross observational level, black, brown, and gray mice tended to be wilder and harder to handle than most albinos, agoutis, and piebalds. But there was little indication that coat-colour genes might usefully serve as markers for functional traits.

Now, although some of the strains studied were clearly different from

each other, there was found to be, nevertheless, great variability within each one. This often resulted in overlap between stocks whose mean scores were widely separated. With complex psychological traits which probably depend on multiple factors, variability is perhaps to be expected, and, as Dobzhansky (5) has pointed out, the designation "hereditary" need not be restricted to characters which show a certain constancy of expression. Such traits as aggressiveness and dominance, for example, while having a genetic basis, can readily be altered by training (9). From the standpoint of analysis, of course, it is more satisfactory to deal with traits which do not vary a great deal within homogeneous stocks, and are fairly resistant to environmental influences. But in the field of behaviour, such traits may be few and far between. Consequently, if psychogenetics is to make any progress, it will be necessary to examine the sources of variability and find methods of reducing it to a minimum.

Obviously, any variability that appears within a given population will depend on two main causes. The first is the genetic background of the individuals making up the population. Since the strains used in the present experiment were all inbred (the criterion for an inbred strain being 20 consecutive brother-sister matings), it is probable that genetic variation was minimal within each strain. None the less, there may have been some. Complete genetic homogeneity is reached by inbreeding only in the limit; furthermore, minor mutations may have occurred spontaneously from time to time. The effects of such slight genetic variation on behavioural phenotypes are difficult to determine, but sometimes they may be important. By way of illustration, Scott (20) has shown that under weak illumination *drosophila* will crawl to or away from a light, depending upon the possession of red or white eyes, a difference conditioned by a single gene. Under threshold conditions, in other words, a slight structural difference may have a greatly magnified effect at the behavioural level (21). Similarly, with the naïve animals used in the present experiment, it is possible that slight environmental disturbances (noises, movements, etc.) may have interacted with minor genetic differences so as to produce considerable variation in behaviour.

The second main cause of variation is the environment. Conditions of testing were naturally made as constant as possible for all subjects, but it is not unlikely that conditions of rearing varied somewhat. Slight differences in handling, maternal care, position of living-box in relation to light, and other such factors, may have produced lasting effects.

Now, since some of the most highly inbred stocks showed as much variability as less inbred ones, it is doubtful if further inbreeding would result in appreciably greater within-strain homogeneity. Also, as we have pointed out, the testing-environment of the subjects was held con-

stant and thus could not have been responsible for much of the variance. Consequently, it is probable that the early environment of the subjects was the most important source of variation. This being so, it would seem desirable to control it by raising all animals under as similar conditions as possible. Perhaps the best method of doing this would be by rearing all subjects in a "free" environment, providing a wide variety of stimulation from a very early age. This should result in a heightened threshold of reactivity in the animals, and as a consequence the effects of some unusual stimulus or experience on any particular animal should be minimized. There is no reason to suppose, of course, that strain differences obtained with naïve animals will necessarily hold after such treatment. But new differences which are more stable and less variable within strains might well appear, and these would be of equal interest. Furthermore, they should be more easy to analyse genetically.

Variability is not the only problem in this area of research. Another is that of choosing simple and unitary traits. To a large extent, this is a matter of trial and error. However, if the assumption is made that the more unitary the trait is, the more simple will be its genic basis, then an independent check will be available on the suitability of any one chosen for examination.

From a genetic analysis of simple behavioural characters, psychogenetics should eventually be able to predict the manner of transmission of more complex traits compounded of several simple ones, and then check the prediction against empirical data. Such an undertaking is, however, a long way from fulfilment. Before any real progress can be made, a great deal of work will have to be done. The experiment presented above represents an initial step in such a programme.

Summary

The above experiment was a preliminary step in a programme of research designed to study the genetics of behaviour in mice. Specifically, an examination was made of three behavioural traits, food-drive, emotionality, and exploratory activity, in 14 different inbred mouse strains, and one hybrid strain. Significant differences in each of these characteristics were found between a number of the strains tested, indicating that they have a genetic basis. Several problems in this area of research were discussed briefly.

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NOTICE TO CONTRIBUTORS

BEGINNING with Volume 8, No. 1, in March, 1954, the style of bibliographical reference in the *Canadian Journal of Psychology* will be that now adopted in all APA journals, and described and illustrated in Section 7 of the *Publication Manual of the American Psychological Association* (Supplement to the *Psychological Bulletin*, Vol. 49, No. 4, Part 2; July, 1952).*

We are grateful to the University of Toronto Press for their willingness to make the readjustments involved in this departure from the style of Volumes 1 to 7. And we feel that the change will be welcome to those of our contributors who also publish in APA journals, to say nothing of the editors, who have had to cope with a bewildering variety of citation methods, often by retyping a whole bibliography.

Uniformity in literary style is not sought for in the *Journal*, but uniformity in such technical matters as references is indispensable. And we ask all contributors from now on to make sure, before submitting a manuscript, that the list of references conforms in every particular to the rules given in the *Manual* mentioned, and exemplified in all current APA journals. Attention to the suggestions about typing practices, footnotes, tables, etc., in this and similar manuals would also save much time for authors and editors alike.

THE EDITOR

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A NOTE ON GEORGE BERKELEY

GEORGE A. FERGUSON
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GEORGE BERKELEY was born in the county of Kilkenny, Ireland, in 1685 and died at Oxford in 1753. In view of the current revival of interest in Berkeley, and the relevance of his thought for modern psychology, the bicentenary of his death should not pass without comment. The significance today of Berkeley's thought does not reside in its originality in relation to what we now know; instead, our present knowledge is an instrument for attaining a fresh interpretation of Berkeley, and an enriched understanding both of his problems and of our own. He himself would have grasped fully what I mean to convey here. In a letter to Samuel Johnson, the American philosopher, we find the statement: "I do not pretend that my books can teach the truth. All I hope for is, that they may be an occasion to inquisitive men of discovering truth, by consulting their own minds and looking into their own thoughts."

The new look at Berkeley is clearly attested by the number of books and articles on him which have recently appeared or are about to appear. These include *The Complete Works and Surviving letters of George Berkeley*, edited by T. E. Jessop and A. A. Luce (Nelson, Edinburgh, 6 vols., Vols. V and VI in preparation), *The Life of George Berkeley*, by A. A. Luce (Nelson, Edinburgh, 1949), and *Berkeley: Philosophical Writings*, by T. E. Jessop (Nelson, Edinburgh, 1952). Among books expected for publication before March, 1954, are *The Philosophy of Berkeley*, by G. M. Warnock (Penguin), and *The Unconscious Origins of Berkeley's Philosophy*, by J. O. Wisdom (Hogarth). Both the *British Journal for the Philosophy of Science* (Edinburgh) and the *Revue Internationale de Philosophie* (Brussels) have recently devoted complete issues to a consideration of Berkeley's contributions to philosophy, science, and the philosophy of science, and there are numerous other recent publications.

Why this revival of interest in Berkeley? It results in part from the attempt by physicists to understand in philosophical terms what modern physics is about. This attempt was stimulated by Ernst Mach, who revived certain Berkelian forms of thought in the discussion of modern physics, and who influenced the thinking of Frank, Schlick, Heisenberg, and others.

But, while the thinking of many modern physicists is in the direction of Berkeley, the thinking of many contemporary psychologists is con-

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trarywise. Many psychologists today regard propositions about "mind" and "consciousness" as either meaningless or unnecessary, and in either case not fit subjects for scientific enquiry. The body-mind problem is dissolved in the denial of the concept of "mind." This metaphysical climate results from the reduction by psychologists of nineteenth-century scientific materialism to its final implications. I hesitate to call this a *reductio ad absurdum*, although it appears so to me. We have reached a paradoxical situation, in which some modern physicists are attempting to reduce physics to a "psychology," regarded as the study of that which is given in the experience of the physicist, the only "reality" being sense impressions, all else being constructs of the "mind"; while at the same time many psychologists attempt to reduce psychology to a "physics," regarded as the study of "material things," these being the only "reality." Here with intent I have confounded the meaning of terms to make more vivid the existing disorder. Let the reader unravel these as an exercise in Berkelian thought. When faced with these singular developments—the "mind's" espousal by the physicist and its divorcement by the psychologist, physicist-become-psychologist and *vice versa*—I have the distinct impression that I have found my way not only into the wrong pew but into the wrong kirk.

I should like now to point to the relevance of three features of Berkeley's thought for psychology today. Throughout Berkeley's work we find repeated emphasis on what we have come to call the phenomenological approach to the physics of his time, and particularly to the conception of motion. He argued for direct observation of phenomena themselves, and a cleansing of the mind of the meaningless terms, abstract ideas, and occult qualities which becloud thought, dictate what we observe, and inhibit scientific progress. His observations in this area (such as his comment, "We first raise a dust, and then complain we cannot see") are as relevant to twentieth-century psychology as they were to eighteenth-century physics.

Berkeley emphasized the distinction between the phenomenological, experimental or intuitive—these terms in my vocabulary being synonymous—and the postulational. This distinction is of high value, obviating much confusion in the use of terms. Consider the statement, "The appearance is the reality." This statement is an attempt by Berkeley to say what he means by the term reality. It is definitive. It is not an ontological postulate. Reality as used here has a phenomenological referent and is defined denotatively in terms of that which is immediately given in experience. If the term "reality" is assigned in this context a postulational referent, something beyond the "appearance," the statement becomes meaningless, and falls in precisely the same class as the state-

ment, "Electrons are pink." The distinction which Berkeley emphasized is a simple one, but of the greatest importance in the history of human thought. Failure to distinguish between phenomenological and postulational concepts gives rise to much bewildered writing by psychologists today. No day passes but I encounter misinterpretations, faulty arguments, and fractures of communication resulting from disregard of this distinction, which requires continual re-emphasis.

Of great importance for us now is Berkeley's denial of essentialism in science. Berkeley assigned an important role to theoretical models in science, referring to them as "mathematical hypotheses." The value of theory for Berkeley resided entirely in its use as a tool for deducing certain results amenable to observational or experimental test. He argued that different "mathematical hypotheses," while appearing to contradict each other, might yield the same result in relation to observational or experimental findings, and might equally serve the purposes of science. He protested vigorously against the essentialist doctrine that theory was descriptive of, or in any sense isomorphic with, any postulated "real world" behind that which was directly observable. While modern physics accepts the Berkelian position, much modern psychology does otherwise. Much psychological theory today, including that on the neurophysiological correlates of behaviour, is coloured in its formulation and interpretation by essentialist doctrine. A complete rejection of essentialism in psychological theory construction would, in my opinion, disperse the "occult quality" which adheres to many postulational concepts in psychology today.

To elaborate and expand these few observations on Berkeley is a great temptation, but space does not permit me this indulgence. In concluding I will add only this. Before his death Berkeley began to despair of the ability of many of his contemporaries to fully understand the nature of his thought. Were he to speak to us today as psychologists, across these two hundred years, he would repeat much that he had said and express much the same despair.

CORRESPONDENCE IN THE RATINGS OF DISPARATE ENTITIES

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It is a familiar observation that individual raters of personality traits tend to give inflated, average, or deflated ratings in comparison with the judgments of the whole group of raters, and that this tendency persists in ratings of the same or similar qualities in other situations.

This tendency is not confined to rating of traits. For example, if ratings of the clarity of visual, auditory, and kinaesthetic images by the same subjects are examined, a significant correspondence is noticed, as shown in the following coefficients of correlation obtained in a study of memory and mental imagery (1):

Visual and auditory imagery ratings $r = .73 \pm .05$
Auditory and kinaesthetic imagery ratings $r = .74 \pm .05$
Kinaesthetic and visual imagery ratings $r = .77 \pm .05$

These coefficients indicate a tendency towards consistency in exaggeration or minimization of the estimates of clarity.

Problem

In this study, consideration is given to the correspondence found in ratings of entities without apparent community. Three rating scales were administered to a group of 131 female subjects who were students in a teacher-training institution. The ratings given by them for the pleasantness-unpleasantness of 80 isolated words are compared with: (*a*) the tendencies observed in the same group when they rated their liking or otherwise for a variety of situations in an opinions test; (*b*) the degree of enjoyment they anticipated from a number of occupations in the field of education which were listed in an interests test, and (*c*) their ranking on an aptitude test for elementary school teachers-in-training.

Rating Scales Used

(1) *Opinions Sub-test*

In the preparation of the author's *Aptitude Test for Elementary School Teachers-in-Training* (2), in future referred to as ATEST, approximately 600 items of a so-called "Opinions Sub-test" were given trial. It is not possible to give typical examples of the items which ranged over so many fields, but a general idea may be gained from the following discarded items:

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Indicate your opinion of each of the following situations, etc., by writing 1, 2, 3, 4, or 5 in the brackets. These figures carry the following meanings:

1, You are extremely favourable to it (him, her, or them); you regard it highly; you are very fond of it.
2, You are fairly favourable to it; you like it fairly well.
3, Your opinion is neither favourable nor unfavourable; you neither like nor dislike it.
4, Your opinion is rather unfavourable; you are inclined to dislike it.
5, You are extremely opposed to it; you detest it.

( ) Engaging in an occupation which is routine in character.
( ) Trying to converse with an extremely shy person.

Items were selected for the test by the following procedure. The highest and lowest quarters of a group of teachers-in-training, as determined by average practice-teaching marks during the period of training, were used as criterion groups. An item was retained if: (*a*) the pattern of responses was reasonably consistent for a succession of small groups containing from 16 to 25 superior students, (*b*) the pattern of responses was reasonably consistent for a succession of small groups of inferior students, and (*c*) there was a reasonably consistent difference between the patterns of the superior and inferior groups. The 22 items of which the Opinions Sub-test consists are the discriminating residue.

Before any items were rejected because of invalidity, it was noticed that the total number of "1" ratings was associated positively, and the total number of "3" ratings negatively, with success in practice-teaching. The correspondence, by no means large, was more marked when the number of "3" ratings was subtracted from the number of "1" ratings. A scrutiny of the scoring keys (for male and for female students) in the *Manual* of ATEST will reveal that for the valid residue this property of the "1" and "3" ratings is present.

(2) *Interests Sub-test*

Another sub-test of ATEST, the Interests Sub-test, was designed to give an indication of interest in certain occupations in the field of teaching, using a four-point scale. The directions given are as follows:

Below is a list of occupations in the field of teaching. In the brackets print:
T, if you believe that you would thoroughly enjoy the work
R, if you believe that you would like it fairly well
N, if you believe that you would not care for it
X, if you are not in a position to express an opinion at present.

The Interests Sub-test items were validated in the same manner as those of the Opinions Sub-test.

At the outset, it was obvious that correspondence existed between the number of "T" ratings on the Interests Sub-test and the number of "1" ratings on the Opinions Sub-test.

(3) *A Word Test*

To throw some additional light on the matter, a Word Test was prepared, to be rated for pleasantness-unpleasantness on a five-point scale. In the preliminary validation, 100 isolated words were presented to 42 female subjects with the following directions:

> This is an exercise to find out how accurately you can discriminate between words. Some words are pleasing to look at or to hear. Some are ugly. Others are neither pleasant nor unpleasant. Indicate the words which you like, are neutral to, or dislike, by writing in the parentheses:
>
> 1, if you like the word very much
> 2, if you like it fairly well
> 3, if you are indifferent to it
> 4, if you are inclined to dislike it
> 5, if you dislike it very much.
>
> Try to keep apart in your mind the word itself and the thing it represents. Thus, some may find the word "skunk" pleasing although they may dislike the animal.

Between the total ratings for each word, for each of two randomly constructed groups of 21 subjects, the Pearson *r* coefficient of correlation was .96.

Eighty of the original 100 words were chosen for the final test. Of these, 16 had the characteristic rating "1"; 16 had the rating "2"; 16 had the ratings "3" and "4" respectively. All of the 16 most unpleasant words were not characterized by the rating "5" since there was a tendency to avoid "5," but they constituted the most distasteful fifth of the list. The 80 words were divided into 16 equal groups, each containing a representative of the five classes, so placed as to form varying sequences.

Results

A group of 131 female subjects was used for the present investigation. They had previously taken ATEST, and their ratings were available on the Opinions and Interests Sub-tests. When the Word List of 80 isolated words was presented to the group for rating on the five-point scale, there was nothing to indicate that they comprised 16 five-word groups. The reliability coefficients for the number of "1" ratings and the number of "3" ratings (in which we shall be chiefly interested) were .83 (N = 131) and .80 (N = 131) respectively.

Four sets of percentiles were calculated, for the number of "1" ratings, "3" ratings, and "5" ratings on the Opinions Sub-test, and for the "T" ratings ("thoroughly enjoy") on the Interests Sub-test. Table I gives the median and quartile percentiles, based on the numbers of 1's in Opinions and T's in Interests, for the highest and lowest approximate quarters of the subjects in respect of the numbers of 1's in Words.

From the data in Table I it is apparent that subjects who found in a

TABLE I

RANKINGS ON 1's IN OPINIONS AND T's IN INTERESTS, FOR HIGHEST AND LOWEST SUBJECTS ON 1's IN WORDS (N = 131)

	Percentiles based on numbers of 1's in Opinions		Percentiles based on numbers of T's in Interests	
	Highest 33 subjects in respect of 1's in Words	Lowest 35 subjects in respect of 1's in Words	Highest 33 subjects in respect of 1's in Words	Lowest 35 subjects in respect of 1's in Words
Q_3	82	69	87	65
Median	62	36	62	42
Q_1	29	15	38	12

list of 80 isolated words a relatively large number which they liked "very much": (*a*) tended to be "extremely favourable" to the situations in the Opinions Sub-test, and (*b*) tended to the belief that they would "thoroughly enjoy" certain occupations in the field of education. In contrast, those who found in the list of words relatively few which they liked very much, tended: (*a*) to abstain from the expression of an extremely favourable opinion of the situations, and (*b*) to abstain from the expression of the belief that they would thoroughly enjoy certain occupations in the field of education.

In Table II the percentile rankings on opinions are compared for those subjects decidedly above average, and those decidedly below average, in their degree of "indifference" to the 80 words. Since there was no neutral step on the four-point scale used in the Interests Sub-test, the comparison is confined to the degree of neutrality ("3" rankings) in the Opinions Sub-test. It is a reasonable inference from Table II that neu-

TABLE II

RANKINGS ON 3's IN OPINIONS FOR HIGHEST AND LOWEST SUBJECTS ON 3's IN WORDS (N = 131)

	Percentiles based on the numbers of 3's in Opinions	
	Highest 30 subjects in respect of 3's in Words	Lowest 32 subjects in respect of 3's in Words
Q_3	83	71
Median	68	36
Q_1	41	19

trality towards the words was associated with neutrality towards the situations described in the Opinions Sub-test.

Table III compares the percentile rankings on Opinions of those subjects who were above or below average in their expression of "dislike" or "extreme dislike" of the words. (Since there were few N's in the

TABLE III

RANKINGS ON 5's IN OPINIONS FOR HIGHEST AND LOWEST SUBJECTS ON 5's IN WORDS (N = 131)

	Percentiles based on the numbers of 5's in Opinions	
	Highest 30 subjects in respect of 5's in Words	Lowest 31 subjects in respect of 5's in Words
Q_3	86	75
Median	63	44
Q_1	36	25

Interests Sub-test ratings, they will not be considered.) The results in Table III indicate that subjects who expressed "dislike" of the words more frequently than average were more inclined to express "dislike" of the situations described in the Opinions Sub-test than were those who indicated dislike of the words fewer than the average number of times.

Correspondence with ATEST

In describing the construction of the Opinions Sub-test, mention was made of the fact that the difference between the numbers of "1" and "3" ratings in the preliminary items of the Opinions Sub-test had a small but quite obvious correspondence with the criterion, success in practice-teaching. (It will be recalled that this correspondence was noticed before validation of the items reduced their number to one-thirtieth.) One is prompted to inquire whether a similar correspondence exists between the ratings of the aesthetic pleasure aroused by isolated words and the original criterion. Since practice-teaching marks for the 131 female subjects in question are not available, the complete ATEST, which was validated against practice-teaching marks, will be used instead.

Description of ATEST

Parts I to V of ATEST are pencil-and-paper tests. Since it has been shown that two of these parts, the Opinions and the Interests Sub-tests, have a linkage with the test on the feeling-tone aroused by isolated

words, no surprise will be created by any additional demonstration. Part VI (Performance), the most powerful gun in the aptitude battery, is essentially a concealed interview. The demonstration of any linkage between Part VI and the Words Test would be of significance. The remaining part, Part VII, is a condensation of the subject's secondary school record. In obtaining the final ATEST percentile, the percentile derived from Parts I to V, the percentile from Part VII, and twice the percentile for Part VI are added together.

TABLE IV

PERCENTILE RANKINGS ON PARTS I–V, PART VI, AND WHOLE ATEST, FOR HIGHEST AND LOWEST SUBJECTS ON RATING IN WORDS: 1's, 3's, AND DIFFERENCE BETWEEN 1's AND 3's (N = 131)

	Parts I–V of ATEST			Part VI of ATEST			ATEST		
	Q_3	Med.	Q_1	Q_3	Med.	Q_1	Q_3	Med.	Q_1
(*a*) Highest 33 in respect of 1's in Words	80	44	20	78	53	25	76	55	28
Lowest 35 in respect of 1's in Words	74	29	13	80	55	34	67	50	25
(*b*) Highest 30 in respect of 3's in Words	74	30	9	83	46	20	82	45	22
Lowest 32 in respect of 3's in Words	76	44	29	78	64	50	76	60	43
(*c*) Highest 33 in respect of 1's *minus* 3's in Words	84	48	29	80	66	50	79	69	48
Lowest 33 in respect of 1's *minus* 3's in Words	71	25	8	76	43	21	64	43	22

Table IV gives the median and quartile percentiles on Parts I-V, Part VI, and ATEST as a whole, for subjects in the highest and lowest approximate quarters in respect of (*a*) the numbers of "1" ratings, (*b*) the numbers of "3" ratings, and (*c*) the difference between the numbers of "1" and "3" ratings in the Word Test.

It is suggested that the data reported in Table IV be examined in conjunction with those of Table V. In Table V are given the percentages of subjects of the highest and lowest approximate quarters, in respect

TABLE V

PERCENTAGES OF SUBJECTS OF HIGHEST AND LOWEST QUARTERS, IN RESPECT OF 1's, 3's, AND 1's MINUS 3's IN WORDS, FOUND IN CERTAIN PERCENTILE RANGES ON PARTS AND WHOLE OF ATEST

	Parts I–V of ATEST		Part VI of ATEST		ATEST	
	PERCENTAGE IN PERCENTILE RANGE					
	75–99	0–24	75–99	0–24	75–99	0–24
Highest 33 in respect of 1's in Words	30	27	24	15	24	21
Lowest 35 in respect of 1's in Words	23	40	31	20	20	23
Highest 30 in respect of 3's in Words	23	43	30	33	30	37
Lowest 32 in respect of 3's in Words	25	22	28	13	25	13
Highest 33 in respect of 1's *minus* 3's in Words	33	15	36	6	33	9
Lowest 33 in respect of 1's *minus* 3's in Words	18	45	27	30	18	33

of 1's, 3's, and 1's minus 3's in Words, falling in the percentile ranges 75–99 and 0–24 for Parts I-V, for Part VI, and for ATEST as a whole.

From Tables IV and V it is inferred that subjects who used a relatively high number of 1's in Words tended to make higher scores on Parts I-V than did those who used a relatively low number. There is no evidence that a tendency to use a high or a low number of 1's in Words was linked with the degree of success in Part VI. There is a suggestion that the number of 1's used in Words is positively related to success in ATEST as a whole.

It is inferred that subjects who used a relatively low number of 3's in Words tended to score more highly in Parts I-V, Part VI, and ATEST than did those who used a relatively high number.

It is inferred also that subjects whose differences between the numbers of 1's and 3's in Words were relatively high tended to score more highly in Parts I-V, Part VI, and ATEST as a whole than did those whose differences were relatively low.

Summary and Comment

One hundred and thirty-one female subjects rated the pleasantness-unpleasantness of 80 isolated words on a five-point scale. The instructions given were such as to confine as far as possible the affectivity of the words to their visual and auditory impacts. Maximum pleasantness was denoted by the rating "1"; maximum unpleasantness by "5."

It was found that those subjects who tended to favour particular ratings in the case of words showed a corresponding tendency when they were: (*a*) expressing their liking or otherwise for a variety of situations, and (*b*) estimating the degree of enjoyment they anticipated from a number of occupations in the field of education.

One would hardly predict in advance that those who claim to find more than the average amount of pleasantness in words, as such, would also look forward to deriving more enjoyment from characteristic occupations of school teachers than do those who find the words less pleasant. Nor is the writer aware of any psychological findings on which such a prediction might be based. It seems obvious, however, that the correspondences found must reflect some aspect of the rater's personality. As the writer has stated elsewhere (3) in reference to the Interests Sub-test: "It should not be thought that the sub-test gives solely a measure of the student's degree of warmth towards occupations in the field of elementary school-teaching. . . . Under the actual conditions of validation [on groups all of whom had chosen the teaching profession] it would seem that two factors were operative: one, the content of the item . . . the other, the temperament of the student."

It is accordingly suggested that a consistent tendency to rate disparate entities favourably, indifferently, or unfavourably, represents an aspect of personality whose distribution and relation to other traits might well be investigated. And further, that the existence of such tendencies should be borne in mind when we are interpreting scores on such instruments as Interest Inventories.

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ON THE THEORY OF THE FIGURAL AFTER-EFFECT[1]

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THIS NOTE will attempt to give a theoretical interpretation, in terms of the model of Osgood and Heyer (6), of two aspects of the figural after-effect.

The main objections so far voiced against the Osgood and Heyer theory have been made by K. R. Smith (7), and among his objections—some of which appear relatively unimportant—the most important is perhaps the apparent inability of the so-called statistical theory to deal with the effect known as the Plateau Spiral. The attempt will be made to show that this is indeed partially interpretable within the statistical theory.

The second aspect—less adequately dealt with—refers to an experiment carried out by the author (1), in which it appeared that if, under specified conditions, the test-figure and inspection-figure were superimposed, thus giving an "illusion" figure, precisely the opposite effect was obtained to that found under figural after-effect conditions.

THE OSGOOD AND HEYER STATISTICAL THEORY

Without wishing to give a detailed explanation of this already well-known theory, it would be as well if a recapitulation of its "explanatory" technique were given to cover the simplest sort of after-effect situation. For details of the six postulates which were derived mainly from the work of Marshall and Talbot (5), reference should be made to the original paper.

Osgood and Heyer were able to show that if an inspection-figure (I) is observed for some period of time and is then followed by a test-figure (T), a shift in maximal excitation, and thus in the contour lines, can be systematically predicted. Thus in Figure 1, if I_a represents the hypothetical distribution of adaptation in the on-off processes in area 17 immediately following prolonged inspection of a contour I, then I_b may be taken to represent the actual state of adaptation when the test-figure is presented. Differential recovery from fatigue during the interval before presentation of the test-figure has flattened the original curve I_a. Then if the test-figure falls objectively to one side of the inspection-figure, the curve T_a, which would theoretically be produced without a

[1]I would like to acknowledge the helpful suggestions made by Mr. A. E. Earle of the University of Bristol, and his kindness in giving me access to his unpublished experiments on the Plateau Spiral and other visual illusions.

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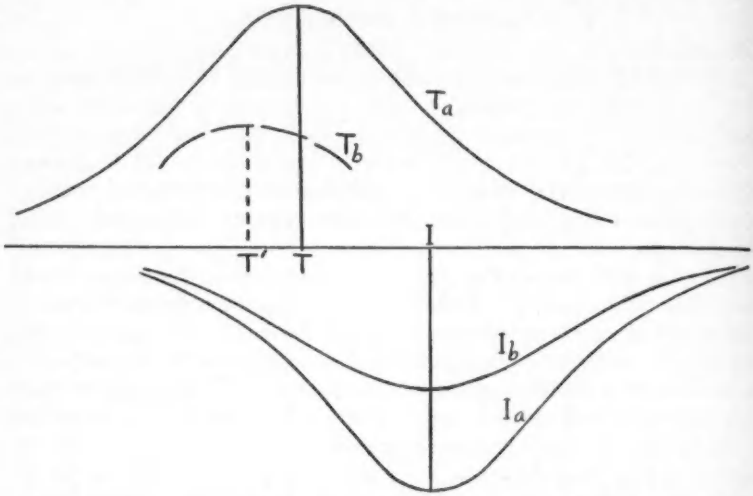

FIGURE 1. The statistical theory. The adaptation curve I_a is flattened to I_b before the test-figure is presented. Thus the theoretical curve T_a is flattened and displaced to T_b.

preliminary inspection period, is flattened to T_b. At the same time the displacement of T from I further displaces the point of maximal excitation from T to T'.

The Osgood and Heyer theory has been recapitulated in the simplest possible manner, and the only criterion here considered is that of the change of size of the test-figures used. The contour displacement is thus the important factor and the other effects noted by Köhler and Wallach (4) can, we may suppose, easily be accounted for (3).

THE PLATEAU SPIRAL EFFECT

The "Plateau Spiral" can be shown to be a central phenomenon which gives an after-effect comparable to the classical figural after-effect. If the spiral is presented to the subject in such a way that he fixates the central point, then he is aware, when the spiral is rotating, of an effect of movement towards him (assuming the rotation is in the appropriate direction). Then if the rotation is stopped and the subject continues to fixate the central point, he reports an apparent movement of the spiral in the opposite direction. This is the situation that Smith (7) regards as inexplicable by the statistical theory.

Now if the inspection period of the spiral is a period of rotation that causes the subjective impression of an unwinding towards the observer,

this is objectively accompanied by a movement outward of the contour lines of the spiral on the retina which will be recorded in area 17. Thus when the inspection-rotation is stopped there will be an I-curve recorded in area 17 (cf. Figure 1) which will flatten from differential recovery from fatigue (this is unnecessary to the theory, and if the change from inspection to test-figure is assumed to be instantaneous the argument still holds), and this curve unlike the static case will have a differentially changing or asymmetrical gradient between any two lines of the spiral. This will clearly be caused by the movement of the contour lines across the retina and will be represented by a greater adaptation near the contour lines on the peripheral side. The latency in the fatigue effect must be such as to place the minimum effect just on the central side of the contour line. Thus when the rotation is stopped the differential excitability set up by the static spiral figure will be symmetrical about the contour lines and the superposition of the I-curve and the T-curve will have the effect of a displacement toward the centre of the spiral. Thus is set up, in the same way as the static displacement of the classical figure after-effect, the subjective impression of a rotation toward the centre. It seems that the same form of explanation could be applied to the "Waterfall" effect. This could at least form the basis of a dynamic displacement.

Simultaneous Presentation of Test and Inspection Figure

The work referred to here is the author's own (1) and the conditions of the experiment were quite simple. The test-figure was composed of two phenomenally equal squares placed at equal distances either side of a fixation point. The inspection-figure which was superimposed on this test-figure was made up of two circles, whose centres coincided with the centres of the squares; one circle completely *encircled* its appropriate square, while the other circle was completely enclosed by the other square. Two such combined figures were used, one displaced horizontally either side of the fixation point, the other vertically displaced.

The effects obtained under these conditions were precisely the opposite to those obtained under figural after-effect conditions. Thus the square encircled by the larger circle was seen as larger on every occasion by thirty different observers viewing the figure under carefully controlled conditions.

In the above experiment the conditions are clearly different from those pertaining in the figural after-effect situation in so far as no adaptation took place since no inspection period was entailed. Thus the two stimulus figures merely had an excitatory effect on area 17. This effect would clearly be greater for the larger circle. Now, it is not certain that this is sufficient condition, except perhaps within narrow limits,

for the seeing of one square as bigger than the other, although it would appear to cater for the Muller-Lyer illusion also, at least within certain limits. When other factors, such as "experiential" cues, are involved, presumably other cortical areas, such as 18 and 19, are brought into operation and complicate the issue. This is not in itself a satisfactory interpretation of the results, which indeed involve the finding of a neurological "explanation" for the so-called visual illusions, but it does appear to exemplify all that is involved from the viewpoint of the statistical theory. It would need, one supposes, to be supplemented by further experiment to establish the limits within which such "illusions" hold, and further theoretical considerations regarding "attention." However, it may be that within certain narrow limits the inability to discriminate sufficiently the effects of the two contour lines will be sufficient to cause the effect noted. This unsatisfactory position at least has the merit of suggesting some fairly obvious experiments.

Towards a Theory of Central Vision

It seems worthwhile to add a few speculative and very general comments. There is a marked tendency to extend the existing investigations of the figural after-effect to other "central" visual phenomena such as "apparent movement." While it is not supposed that "central" vision is wholly independent of retinal factors any more than it is confined to area 17, it seems increasingly possible that we may be able to formulate explicitly those visual factors that are *primarily* functions of area 17, and thus gradually to build more explicit models regarding the function of the cortex as a whole.

Further to the above remarks it seems for example that the effect—from the standpoint of the artificially isolated visual system—of "apparent" and "real" movement are the same. The distinguishing mark between them, as with many other visual effects, will involve other cortical areas. It is presumably in terms of cues other than the actual "moving" stimulus that the two cases are discriminated, and when these are absent discrimination becomes impossible. The so-called "pendulum effect" (2) admirably illustrates the dominating influence of these other cortical areas in giving an interpretation of those visual phenomena which the visual system itself simply records. The problems of "constancies" might also be approached in the same way.

All that has been said is meant only to indicate the "way the wind is blowing" in matters of vision. The near future should yield more general visual models even than that of Osgood and Heyer, although I believe that theirs is perhaps an admirable foundation. As our neurological knowledge increases, and it is increasing fast, we are surely

approaching a stage for the integration into one general theory of all the effects named above, and more. Thus our neurological models of behaviour—for example that of Hebb (3)—should become more comprehensive and detailed.

Summary

This paper has aimed primarily to show that Smith's objections to Osgood and Heyer's visual model can be partially met, at least with respect to the important matters of the Plateau Spiral effect and the "waterfall" illusion. A brief comment is made on some results, obtained in the case of "illusion" figures, which are a limiting case of the figural after-effect. The concluding remarks express the belief that large scale integrations in visual theory are in the offing, and briefly suggest the course which such integrations may take.

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THE RELATIONSHIP BETWEEN RATE OF REVERSAL OF FIGURES OF REVERSIBLE PERSPECTIVE AND EMPATHY[1]

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Introduction

The perception of another person's behaviour, particularly when repeated over an extended period of time, invariably leads to some conclusions about the perceived person's personality. These conclusions may, in varying degrees, coincide with the perceived person's view of himself. When such coincidence is exact it may be said that the perceiver understands the other as the other understands himself. It is the purpose of this experiment to enquire into the perceptual aspect of the process by which such understanding, or its approximation, is achieved.

The central hypothesis put forward is as follows: the degree of understanding achieved varies concomitantly with the degree to which the configuration constituted by the other's behaviour is *attended to* during perception. To test this hypothesis the following assumptions are made:

1. That the personality of an individual is revealed in his expressive and non-expressive behaviour, and may be understood by another (as he himself understands it) through the perception of this behaviour.

2. That continued attention to the behaviour configuration indicates that perception is being dominated by sensory stimulation, and hence that the perceiver is able directly to apprehend the personality of the other through his behaviour.

3. That when, on the other hand, attention fluctuates, "organismic" or "central personality" factors tend to dominate perception, so that the behaviour is perceived within frames of reference peculiar to the perceiver, and apprehension of the other's personality is to that degree distorted.

4. That the rate of fluctuation of attention in a given individual is relatively constant, and is characteristic of the individual in all perceptual situations.

Method

A modified version of the rating scale used by Dymond (1) as a measure of empathy was used to obtain a quantitative assessment of the understanding the subjects had of one another. Eight figures of re-

[1]This is an abridged version of a thesis submitted in 1950 in partial fulfilment of the requirements of the Master of Arts Degree in the Department of Psychology, University of Toronto. I wish to thank Dr. W. Line and Dr. Morgan Wright for their assistance with the work.

versible perspective were used to obtain a measure of each subject's rate of fluctuation of attention. Flugel (3) has shown that the reversal of these figures may most plausibly be attributed to fluctuations of attention.

Subjects

Twenty-nine psychology students served as subjects. Twenty-one of these were undergraduates and eight were graduates, 12 were male and 17 female. Age range was 18–42 years; mean age approximately 23.

The Rating Scale

The rating scale contained the following six traits: (1) self confidence, (2) popularity with others, (3) sense of humour, (4) co-operativeness, (5) friendliness, (6) easy-going nature. All ratings were made on a five-point scale. In a row after each trait appeared the numbers one to five, with the "one" column specified as indicating a "low degree" of the trait in question and the "five" column a "high degree." The scale was divided into four parts and each part of the scale required the subjects to make ratings from a different point of view, as follows:

Part 1. Rate himself on a five-point scale on each of the six traits.
Part 2. Rate other individuals, specified by name, on the six traits.
Part 3. Rate himself on the six traits as he thinks these same individuals would rate him.
Part 4. Rate these same individuals on the six traits as he thinks they would rate themselves.

Each part of the scale was contained on a separate page. Instructions on a face page read as follows:

This is a rating scale on which you are to rate yourself and three other people from different points of view. The way you are to make each of the ratings is specified on each page. Do not deliberate long over any one rating but try to be as accurate as possible. Be sure to complete each page before proceeding to the next.

On each following page the point of view from which the rating was to be made, and the person to be rated, were specified.

The Figures of Reversible Perspective

Eight figures were used: (1) outline of a plain table, (2) cube with a section removed, (3) three reversible blocks, (4) reversible folded paper, (5) reversible circles, (6) reversible rectangular block, (7) Beaunis' cubes, (8) Schroder's stair figure. The figures were reproduced in india ink on 4 by 6 inch tachistoscope cards to cover an area approximately 2½ inches square in the middle of the card.

Procedure

One week before administration of the rating scale, each subject was asked to indicate how well he knew each of the other 28 members of

the class on a five-point "Degree of Acquaintance Form." This was done after the class had been together in lecture and laboratory for two and one-half months. The degree of acquaintance each number represented was specified at the top of each column, thus: 1—only by name, 2—just acquainted, 3—quite well, 4—very well, 5—intimately.

On the basis of the results of this scale each subject was asked to rate and to predict the ratings of three others with whom he had indicated that he was "just acquainted," or whom he knew "quite well." Since it was necessary to compare each individual's predictions of the others' ratings (on parts 3 and 4 of the scale) with the others' actual ratings (on parts 1 and 2 of the scale), all matchings had to be reciprocal. For example, if A rated and predicted the ratings of B, C, and D, he was rated and his ratings were predicted by them as well.

From the completed rating forms a numerical value for the accuracy of each subject's predictions was computed by adding up the number of points' difference between his predictions and the other's actual ratings. For example, if A predicted that B would rate himself "5" on "sense of humour" and B actually rated himself "3," an error of 2 was attributed to A. The total number of such errors in all of one subject's predictions constituted his "deviation score." A "right score"—the number of predictions that were exactly right—was also computed for each subject.

The figures of reversible perspective were presented by means of a tachistoscope designed in the Department of Psychology, University of Toronto. Each figure was exposed for 30 seconds followed by a 30-second rest interval during which a blank white card was visible through the aperture of the tachistoscope. Illumination was provided by an overhead 250-watt lamp. The subjects, taken individually for this part of the experiment, were seated 22 inches from the tachistoscope. They were then shown the first figure, familiarized with the phenomena, and given practice with a spring key, connected to a magnetic counter, which they were to close each time a reversal of the figure occurred. After this practice period the subject was instructed to relax, to observe the figures as they were shown, and to record each reversal by closing the key. They were instructed to observe the figures with a passive attitude, that is, to make no effort either to change the perspective or to prevent it from changing.[2] The seven remaining figures were then shown. Data for the second figure were discarded, this figure being used to give the subject

[2]In the original experiment, passive observation of the figures was followed by repeated observation with an "active attitude" (attempting to change the perspective as rapidly as possible) and a "resisting attitude" (attempting to prevent the figures from changing). It was shown that each of these three attitudes had a significant effect on the rate of reversal.

practice under experimental conditions. The total number of reversals of the last six figures constituted the subject's score.

Results

The Deviation Scores obtained from the rating scale ranged from 19 to 48 with M = 28.14 and σ = 5.8. Right Scores ranged from 7 to 21 with M = 13.59 and = 3.22. The correlation between the two sets of scores was $r = +0.84$.[3] Because of the larger range of the Deviation Scores they were used for most of the statistical computations.

The Right Scores provided a convenient way of determining whether or not the accuracy of predicting others' ratings was greater than could be expected by chance. Since predictions were made on a five-point scale there was one chance in five that any one prediction would be exactly right. Regarding each prediction as an independent event, we would expect that one-fifth or 7.2 of the 36 predictions made by each subject would be exactly right. The obtained mean number of exactly right predictions, however, was 13.59. The difference between the obtained and expected means is significant beyond the 1 per cent level of confidence ($p = .004$). We may conclude, therefore, that the scores represent some predictive ability and not pure chance.

The reliability of the scale was computed by correlating the Deviation Scores for traits 1, 3, and 5 with those for traits 2, 4, and 6. The obtained r equals +0.38 with a theoretical value, as estimated by the Spearman-Brown prophecy formula, of +0.55.

The total number of reversals for the six figures of reversible perspective ranged from 8 to 107 with M = 40.25, and σ = 24.39. To estimate the reliability of the scores the number of reversals of figures 3, 5, and 7 were correlated with the number of reversals of figures 4, 6, and 8. The obtained r equals +0.82 with a theoretical value of +0.90.

Correlating the Deviation Scores and the scores consisting of the total number of reversals gave $r = +0.40$ ($p = .05$). This means that a rapid rate of reversal on figures of reversible perspective is correlated with a large error in predicting the ratings of others and *vice versa*.

Discussion

If the assumptions stated in the Introduction are accepted, these results may be regarded as substantiating evidence for the hypothesis. Although neither the reliability of the rating scale nor the correlation between accuracy of prediction and rate of reversal is high, the results would appear to be of considerable theoretical interest. In his analysis

[3]All coefficients of correlation were computed by the Product-Moment Method.

of attention, Hebb (4, p. 26) argues that ". . . fluctuations of attention which occur point directly to the fundamental importance, in any perception, of nonsensory factors." The results of this experiment suggest that the rate of fluctuations of attention is relatively constant for any one individual, and hence that the relative importance of sensory and nonsensory factors in his perceptions may be relatively constant also.

Dymond's (2) personality appraisals of the extremely accurate and extremely inaccurate predictors on her scale (essentially similar to that used in this experiment) are consistent with the interpretation here made. From the common tendencies in their TAT and Rorschach protocols she makes the following general statements: of the inaccurate group, "introverted, motivated from within, . . . they are most often egocentric in their relationships, using other people for their own purposes, for the feeling of power and status it gives them"; and of the accurate group, "extroverted—being more responsive to promptings from without—they are sensitive to the feelings of others . . . have a great interest in other people." It seems reasonable to suppose that the latter type of person would be more likely than the former to attend to the configuration constituted by another's behaviour, and thus to apprehend the personality to which this behaviour has relevance.

Summary

Two experimental situations were set up to test the hypothesis that "the degree to which an individual achieves understanding of another, as the other understands himself, varies concomitantly with the degree to which the configuration constituted by the other's behaviour is attended to during perception." The first situation involved a rating scale which required each subject to predict how others known to him would make various ratings. The second required the subject to observe figures of reversible perspective and record the number of reversals that occurred. Twenty-nine psychology students acted as subjects. Those who reversed the figures of reversible perspective rapidly were significantly less accurate in their predictions of others' ratings than those who reversed the figures more slowly. This relationship is interpreted as substantiating evidence for the hypothesis.

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THE USE OF PSYCHOLOGICAL TESTS IN PSYCHOSOMATIC RESEARCH[1]

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THE PRESENT RESEARCH PROJECT is in some respects an extension of the duodenal ulcer investigation done at the Kingston General Hospital in 1948 (5). The result of that work was presented at the annual meeting of this Association in 1949, and led us to the conclusion that a group of patients with duodenal ulcer showed certain characteristics on the Rorschach Test which differentiated them from a group of hospital patients suffering from various other somatic disorders. It was found that the Rorschach performance of duodenal ulcer patients was less variable than that of the mixed control group, and both the M:FM and FC:CF ratios were significantly more often unfavourable in duodenal ulcer patients. It was also noted that the three measures of experience balance, as used by Klopfer, typically showed duodenal ulcer patients to have an overtly extratensive orientation superimposed on a basically introversive personality structure.

Some association between specific personality traits and certain organic syndromes has been repeatedly asserted in the psychosomatic literature (2, 4, 6, 7, 8, 11, 13, 17, 20, 25). It has also been explicitly stated in theoretical form by Alexander, who writes that "the physiological responses to different emotional tensions are varied and that consequently vegetative disfunctions result from specific emotional constellations" (1). In opposition to this theory are the findings of Klein (14), Fenichel (10), and Lhamon and Saul (15) who maintain that specific gastro-intestinal illness cannot be correlated with any personality type.

In the present study, carried out at the Maudsley and King's College Hospitals, London,[3] it was postulated that if vegetative disfunctions do indeed result from specific emotional constellations, then it should be

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possible to demonstrate these by objectively verifiable methods. To this end various personality tests were used in order to assess the extent to which groups of patients with various psychosomatic disorders can be differentiated. Our search for psychological correlates of organic illness has various theoretical implications. Although it has already been demonstrated that a particular disease group such as ulcer patients or asthmatics may differ psychologically from a healthy or at least non-psychosomatic control group, it is obvious that such a differentiation does not necessarily argue for psychological specificity. On the other hand, a direct comparison of two disease groups, where the effect of chronic non-specific illness is eliminated, might reveal personality differences associated with a specific disease provided other variables have been suitably controlled. In what is to follow we have, therefore, set out to test the hypothesis that, on a battery of psychological tests, patients with a particular psychosomatic disorder will differ significantly, and in the expected direction, from those with some other vegetative disfunction. We also expected to find the test performance of patients with psychosomatic diseases to differ significantly and predictably from that of a healthy control group. All predictions were based on observations cited in the clinical literature.

Since peptic ulcer is probably the most common of the so-called psychosomatic affections, and one on which a great deal of research has already been done, 30 patients with duodenal ulcer were selected for this study. As a contrast group, an equal number of patients with ulcerative colitis were seen. This group was chosen on the assumption that if the specific nature of an illness has an effect on the patient's personality, as is sometimes maintained, then such an effect should be minimized when comparing two diseases affecting the same organ system. Also previous psychiatric investigators, such as Sullivan and Rehfeldt (23), Brown *et al.* (5), Weiss and English (24), Gildea (11), and many others, have commented on the apparent contrast between the ulcer and colitis personality. The control group consisted of people whose medical history was negative to gastro-intestinal disturbances and who, at the time of testing, were not in need of medical attention. The three groups were equated for age, sex, intelligence, and socio-economic status. Each group included 15 male and 15 female subjects in the age range from 25 to 56 with a general mean of 39 years. The study was confined to outpatients, the vast majority being either employed or otherwise discharging their daily duties at the time of this investigation. The median duration of symptoms was very similar in both patient groups.

In addition to a 30-minute interview followed by a brief intelligence

test, eight other tests and measures were used. Of these, five were objective behaviour tests such as had previously been described by H. J. Eysenck (9) who found these tests helpful in differentiating normals from neurotics. Included were a level of aspiration test, a measure of motor conflict and reaction time, a test of pain sensitivity, the body sway test of suggestibility, and a measure of persistence. Each patient was seen on two occasions. During the second session a continuous measure of changes in galvanic skin resistance was obtained during the administration of five cards of the Rorschach test and five cards of the T.A.T.

All quantitative findings were subjected to analysis of variance. In three of the tests significant group differences in the expected direction were found. Thus primary suggestibility, as measured by the Body Sway Test, was greatest in colitis patients, the difference between ulcer patients and controls not being significant. Again, pain reaction level, that is, the point at which the patient asked that pain stimulation be discontinued, was found to be significantly lower for colitics than for either of the other groups. However, pain perception level, that is, the point at which pain sensation was first reported, was found to be much alike for the three groups. Duodenal ulcer patients tolerated less pain stimulation than healthy controls; the F-ratio being 12.72, significant beyond the 1 per cent level of confidence. These results are in complete agreement with earlier work by Schilling and Musser (22), who used the Hardy-Wolf-Goodell technique of pain stimulation. Lastly, changes in galvanic skin resistance, following ideational and sensory stimuli, tended to be more pronounced in colitis patients, but on only one out of eight G.S.R. measures obtained was the difference significant.

When scores from these three tests were combined by T-scaling, according to the method of McCall (18), all F-ratios so obtained were found to be significant at the 1 per cent level or better. On plotting the scores it was found that the optimal cut-off point discriminated between ulcer and colitis patients with 25 per cent misclassification. A discriminant function analysis carried out on these data yielded the same percentage of misclassification. On comparing scores which significantly differentiated patients from controls, such as reaction time, level of aspiration, and pain reaction, it was possible to discriminate between ulcer and controls with 28 per cent misclassification, and between colitis patients and controls with 13 per cent misclassification.

In summary, the results show that suggestibility, pain reaction, and galvanic skin resistance activity are more pronounced in the colitis than in the ulcer group and the measurable aspect of this difference was suf-

ficiently marked to serve as a basis for significant group discriminations. Similarly, reaction time, level of aspiration, and pain reaction significantly differentiated controls from both patient groups but failed to discriminate between the latter. On all tests where significant differences were found, controls tended to be at one end of the continuum, colitics at the other, and ulcer patients intermediate.

In the qualitative interpretation of these findings it is well to remember that our use of terms such as "suggestibility" and "pain reaction" is operational, and has reference solely to the quantitative variables, that is, the test performances on which they are based. No extension of the terms beyond that scope is presently intended. Nor is it safe, at this stage, to assume that our results apply to samples other than those from which they were derived. With these reservations in mind the suggestion is that colitis sufferers differ from ulcer patients in that they show greater ideo-motor suggestibility, less tolerance for pain, and greater adrenergic responsiveness to psychological stimuli. The suggestibility and pain tests had previously been found to discriminate normals from neurotics, and on both tests the scores obtained by colitics are akin to those previously associated with neurotics. These objective indications appear to be in agreement with clinical impressions of colitis patients as recorded by Paulley (19), Groen (13), Lindemann (16), and others.

Further evidence of specificity was obtained from projective tests, notably the T.A.T. Five cards only were shown and the 90 protocols, in random order, were submitted to another psychologist for "blind analysis." The three groups were identified with better than chance success simply on the basis of what the examiner had gathered from the clinical literature. A thematic analysis of stories was then undertaken and group similarities and differences were noted on an instruction sheet. This was used by four independent judges as a basis for identifying the three groups and here again the classifications arrived at were significantly better than chance expectation at high levels of confidence. The thematic productivity of both patient groups was typically low. Duodenal ulcer patients showed considerable preoccupation with the need for achievement and their inability to relate themselves to others in any personal sense was striking. Colitis sufferers were distinguished by their persistent avoidance of antisocial themes, submission to authority, lack of aggression, and need for emotional support by the family.

On comparing Rorschach protocols of the two groups, the most prominent feature was the colitis patients' emphasis on colour-form (CF) and even pure colour (C) responses. It is of particular interest that the occurence of these uncontrolled colour responses was frequently accompanied by reduced autonomic activity as measured by the G.S.R.

Conclusions

The group differences demonstrated by this study were taken to support the theory that a specific constellation of traits is associated with each of the organic syndromes here considered. To that extent our findings are consistent with Alexander's theory which we set out to test. While we consider it likely that these personality trends—and undoubtedly many more that we have not tested—may predispose an individual to physiological disfunctions culminating in organic lesions, definite proof that we are justified in so extending our results is lacking. We submit, however, that the personality differences found are not ascribable to aspects of non-specific illness and that they cannot reasonably be regarded as resulting from the two diseases concerned.

While no clinical application of these findings is presently contemplated, it is felt that their use in conjunction with psychophysiological studies along lines pursued by Grace, Wolf, and Wolff (12) may ultimately help to throw some light on the mechanisms involved in the psychogenesis of physical disorders.

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MATHEMATICAL TRAINING FOR APPLIED EXPERIMENTAL PSYCHOLOGY

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INTRODUCTION

WE ARE a nation of knob twisters, dial readers, button punchers, lever pushers, and light watchers. We are machine tenders. With knobs, dials, buttons, levers, and lights we govern the behaviour of mechanical and electronic devices which have transformed us into a mechanized society. The machines we tend include radios, electric calculators, automobiles, aeroplanes, warships, radars, wrist watches, time clocks, geiger counters, and printing presses. There are thousands more. Without our superiority in machines we would not have won our recent wars (4).

The quantity and quality of the output of a machine depends directly upon the input, that is, upon the behaviour of the tender. Skilful behaviour usually results in a desired output. Unskilled behaviour can be worse than none: it can be disastrous. The degree of skill on the part of the tender is often studied by dichotomizing it into the interacting facets of speed and accuracy, a process which results in a large aggregation of quantitative data.

While there exists a profusion of terms to describe this study of man-machine relations, the writer feels (for a number of reasons into which he will not enter here) that the most suitable term in current use is Applied Experimental Psychology.[2] Of the 663 members of the Canadian Psychological Association in 1953 (5) certainly not more than 10 (0.01 per cent) could describe themselves as being trained in this highly important field, and for a good reason. The training required to ensure competence is not offered by any of the fifteen psychology departments in Canadian universities.

[1]The opinions expressed in this paper are those of the writer and do not necessarily reflect the views of the Department of National Defence. The writer wishes to acknowledge the helpful comments of Professor G. A. Ferguson.

[2]The term Human Engineering is also commonly used. Other terms, some of which imply a more restricted field of study, include Applied Psychophysics, Applied Psychophysiology, Bio-Mechanics, Biotechnology, Engineering Psychology, Equipment-Design Research, The Human Factor in Equipment Design, Man-Machines Systems Research, Psychoacoustics, Psychological Problems in Equipment Design, Psychophysical Systems Research, Psychotechnology, and Systems Research (7, 9, 13).

The Problem of Communication

It is generally agreed that many problems encountered in this field can be solved only by application of knowledge derived from a number of different sciences. Their solution involves the application of the principles of "medicine, psychology, physiology, anatomy, anthropology, and engineering" (2). Mead (23) states that, "in addition to the engineers who devise a particular type of equipment, there may be the need . . . for motion-and-time-study men, physicians, psychologists, physiologists, and other specialists from the field of the biological sciences." But the colligation and application of pertinent principles and data from these many fields is done by the psychologist. And the people with whom he must be able to communicate most often are the engineers and physicists —people who communicate most effectively by mathematics.

This union of disciplines is admirably demonstrated by Chapanis *et al.* (7) who stated, in connection with the writing of their text: "In addition to our research effort in this field, we are still in the stage of compiling information and methods from all sorts of places: from textbooks of experimental psychology, physics, and motion-and-time engineering; from the journals of these professions, and from many previously classified reports which have not yet seen the light of day." Indeed, over fifty of their selected references are from sources which could in no sense be construed as psychological, including, for example, *Proceedings of the Highway Research Board* and the *Journal of Industrial Hygiene and Toxicology*.

Some Areas Requiring Mathematical Tools

Before considering the type of academic training required to ensure a fairly adequate background for research in applied experimental psychology, it may be well to point out some of the areas of psychology involved.

Let us consider first an important and relatively new area of research in which, to the best of the writer's knowledge, only one Canadian psychologist has taken the slightest interest—that of information theory. The applied experimental psychologist is often confronted with a so-called "system," usually composed of a series of men-machine combinations, through which information, often verbal, must be relayed as quickly and accurately as possible. For example, in civil aviation, such a system may comprise the machine-tending navigator, the machine-tending pilot, and the machine-tending ground-controller. At night, and particularly in bad weather, lives may depend upon the efficiency with which such a system operates. Some knowledge of information theory is a prime requisite to research in this field.

A recent and pertinent paper (24) appeared in a journal which aims

to publish articles of interest to all psychologists, the *American Psychologist*. The writer asked fourteen psychologists whether or not they had read this particular paper. Only one had. Invariably, the reason given for skipping it was that it was "too mathematical," or "too full of formulas." This would doubtless be a blow to Miller, the author, who went to considerable trouble to treat his topic as simply as possible.

But let us not pass by Miller's paper too quickly. Let us examine the bibliography. A rather startling fact emerges. With the exception of the historical reference to Hartley (1928), every one of the thirty-five references, including the well-known systematic presentation by Shannon (26), was published later than 1947. As Miller says, the new field of information theory is currently causing "considerable fuss." But not in Canada; for most of us it is "too mathematical."

We may now glance briefly at sequential analysis.

> Sequential analysis is a method of statistical inference whose characteristic feature is that the number of observations required by the procedure is not determined in advance of the experiment. The decision to terminate the experiment depends, at each stage, on the results of the observation previously made. A merit of the sequential method, as applied to testing statistical hypotheses, is that test procedures can be constructed which require, on the average, a substantially smaller number of observations than equally reliable test procedures based on a predetermined number of observations (28).

What a boon to the weary psychophysicist! And, since "it seems possible that the results of *all learning experiments*[3] can be described in a uniform manner and the results of one experiment compared with the results of others" (25), what a technique for the "learning man"! And how conspicuous by its absence!

But we are not concerned solely with new fields. Some traditional areas of psychological research can be made to yield new information through the use of recently developed, and infinitely sharper, tools. For instance, a method of dealing with psychophysical data has been evolving through the efforts of biological statisticians. It is termed "probit analysis." A recent author (12) describes it as "a statistical treatment of the sigmoid response curve." The author, a biostatistician, states that the underlying principle, that of reducing a response curve to a straight line, was invented by psychologists—Fechner (1860), Miller (1879), Urban (1901), Thompson (1914), and Whipple (1916). Yet, although these early psychologists were fifty years ahead of the biologists in developing the technique, it would seem, as Finney (12) points out, that "psychologists apparently remained unaware that their method had been adapted and refined by biologists." The notable Canadian exception to this statement is Ferguson (11).

[3]Italics inserted by the writer.

The Defence

"Call it what you want, but don't call it psychology," might be the response to the foregoing, despite the fact that nearly all the leaders in this field of applied experimental psychology are well-known experimental psychologists.

"Psychology can get along very well without mathematics" could be a defence for psychologists who have never computed a correlation coefficient, though it is doubtful if such exist. On the other hand, it is beyond doubt that psychologists whose statistical sophistication has never matured beyond the correlation coefficient are still living, psychologically speaking, in the era of Galton and Pearson. And it is largely due to such truncated statistical backgrounds that the field of personnel selection, as worked in by "orthodox psychologists," has evolved no really new techniques in the last twenty years. The current hope of some in this field—that quality control methods will bring about the needed improvement—merely indicates that psychologists themselves are not trained to make advances in a field which they regard as a legitimate area of study.

This willing dependence on others was alarmingly apparent in the response of another psychologist when the writer expressed a wish that he could acquire a stronger statistical background. The response was, "Why worry? You'll always find a statistician lying around." This is just not true. And, if you are sufficiently fortunate to find one lying around, he will be a rare bird indeed who can quickly grasp the psychological problems involved and at once proceed to do your work for you.

"But let's discuss 'real' psychology," some irritated reader may demand. Very well, let's discuss sensation. Specifically, let's spend a moment with vision. One of the monuments in the field of vision was published nineteen years ago by Hecht (15). Unfortunately, it is "mathematical." And so, in part, is the recent and very important review by Bartley (1). Jahn's attempt (18) to compare theories of "certain visual phenomena, namely flicker fusion frequency, brightness discrimination, visual acuity, and instantaneous thresholds as functions of intensity, and light and dark adaptation as functions of time," would be meaningless unless mathematical symbolization was employed. And Luneberg's (22) unique demonstration of the non-Euclidean hyperbolic character of binocular visual space could never have been written. The vision student who cannot understand this randomly chosen sample (we could just as easily discuss colour, or photometry) will inevitably feel inadequate, and will probably devote his time to easier and less rewarding study. The "less rewarding" here is a two-edged sword: less rewarding for the student, and less rewarding, possibly, for the field of vision also.

It is quite true, of course, that much of the literature on vision can be understood without recourse to mathematics. But understanding the work of others, though it may be the major part of the teacher's job, is only a minor part of the scientist's; the other part is contribution. Understanding the principle of the carburettor sufficiently well to know why choking helps to start the motor on a cold morning—this is an achievement of sorts. A vastly superior achievement, a contribution, would be to design a better carburettor. Contribution in the visual field, and understanding too, are becoming increasingly difficult for those without the mathematical tools.

Finally, let us consider the field in which psychology claims a greater knowledge than any other science: that of learning. To many psychologists the terms "learning theory" and "psychology" are synonyms. Here two quotations will make our point. Hilgard (16) thinks Hull's *Mathematico-Deductive Theory of Rote Learning* "the most difficult book ever written by psychologists." And it probably is. Hilgard continues: "The later volume, Principles of Behavior (1943), was originally planned to be called a 'primer' of behavior and is intended to be at a much less difficult level.The complexities which persist in it are of the sort which any serious student of behavior must be prepared to master." And from what is probably the foremost school of learning theory today, the State University of Iowa, we have the following statement (21):

> Ten or twelve years ago (circa 1937) several staff members in experimental psychology at Iowa reached the conclusion that most graduate students were unable to understand some of the articles and books they were assigned to read. . . . Those who had a course or two in calculus fared somewhat better than those who had stopped with college algebra, but the difficulty did not stem entirely from flimsy backgrounds in mathematics. Our students simply had not yet learned to think in quantitative terms, to discover basic relationships between variables, and to use whatever mathematical knowledge they possessed as a tool in scientific analysis and description. The upshot was that we decided to introduce a course. The course does not replace the usual courses in statistics. It is given *in addition to six other courses*[4] (three of them quite advanced).

Iowa would not be so eminent in the learning field today if staff and graduate students had not had such a background.

One could consider other fields. For instance, there is the social psychologist who wishes to evaluate the effectiveness of his propaganda leaflets, and the clinician who attempts to determine the reliability and validity of his projective device. Too often, it is feared, they must defend their methods by rationalizing, as Brower did (3) when he told a group of psychoanalysts that "numeration *per se* is essentially atomistic and tends to be distortional because it leads to over or under-determination

[4]Italics inserted by the writer.

of meaning," and "the correlation coefficient, in order to be rendered meaningful, must be qualified in so many ways as to make the procedure *laborious and complicated.*"[5]

By now the reader may have a little insight into the writer's bias. Applied experimental psychologists *must* have a solid background in mathematical science.[6] And who are applied experimental psychologists? They are simply experimental psychologists who have the task of finding generalized solutions to practical problems. By experimental psychologists we mean those psychologists who conduct experiments, and we include in their ranks all graduate students.

The Training

A brief sample has been given of areas in which experimental psychologists who lack mathematical training will find it difficult to achieve understanding, let alone make significant contributions. And it is only a sample; there are many more such areas. It is not implied, of course, that the beginning experimentalist must be familiar with the mathematical techniques peculiar to each area; such proficiency at the Ph.D. level is neither practicable nor necessary. What *are* required are the basic tools. Given these, understanding in any area can be achieved by diligent application of known principles to new fields. Such diligent application is one of the things for which the research psychologist is paid. The training in known principles, however, is the responsibility of the teaching institution.

Here the writer has the temerity to be specific as to how such basic mathematical knowledge could and should be imparted. The programme suggested is woven into a typical seven-year programme leading to the Ph.D. in experimental psychology: four years to an honours B.A., another year to an M.A., and two years further to a Ph.D. Here it is:

Year	*Course*	*Hours per week*
1st	A. Introductory college mathematics	3
	B. Introductory college physics (with lab.)	3
2nd	A. Introduction to differential and integral calculus	3
	B. Physics of light (no lab.), including a study of optical instruments, photography, spectroscopy, photometry, thermal radiation, refractometers, interference, diffraction, polarized light, and geometric optics; or a similar course in sound, or in thermodynamics. The purpose of this course is to teach the student some	

[5]Italics inserted by the writer.

[6]This bias (and indeed, this paper) was succinctly restated by a non-psychological biostatistician who kindly read the paper and observed: "The little real meat in this could be summarized very briefly, and is too obvious to write a paper about. People who deal with quantities rather than qualities should learn to speak the language."

	fundamental techniques of measurement and control of sensory stimuli.	1
3rd	A. Differential calculus, including the real number system, sequences, series; derivatives of functions of one and several variables, implicit functions; applications to the differential geometry of curves and surfaces	2
	B. Theory of probability	2
4th	Mathematical statistics; a mathematical introduction to statistical analysis, with emphasis on sampling theory and the testing of statistical hypotheses; applications to psychological data	3
5th	A. Experimental design and psychophysical techniques, e.g. Edwards (10), Guilford (14), and Kendall (20)	2
	B. Factor analysis, e.g. Cattell (6)	1
6th	Experimental design (advanced); including methods of increasing the accuracy of experiments; completely randomized blocks and latin square designs; factorial design; confounding; balanced and partially balanced block designs, lattice designs; incomplete latin squares, e.g. Cochran and Cox (8), who present 150 of the most useful experimental designs, and Kempthorne (19) who supplements Cochran and Cox to a considerable degree	3
7th	Variance and covariance analysis (advanced), e.g. Jackson (17)	2

The above courses should end, in every case, with a formal examination. Graduate seminars in psychological theory may properly rely on term papers, but the mastery of tools requires drill, not debate; factual knowledge, not opinion.

The reader may protest that such a course would graduate technicians, not psychologists. If a "psychologist" cannot perform a technically sound experiment, and a "technician" can, then, if their backgrounds in psychology be equal, let's graduate technicians. The plain truth of the matter is that an experimental psychologist with a background like that outlined above will be preferred by any respectable research laboratory to one whose statistical sophistication is limited to knowledge of how to compute a critical ratio.

Another protestation might be that other training areas would have to suffer if this mathematical training were given. It is true that other areas might suffer, but not other training areas. It is high time that curricula were examined in the full light of modern needs. A beginning can be made by tossing out language requirements. No one-year or even two-year course in scientific German or French will enable a researcher to read critically an experimental paper in these languages—if, indeed, he is ever required to read one. And if, as the writer suspects, the sole purpose of the language requirements is to make the Ph.D. just a little harder to achieve, then the purpose is admirably served by the above programme.

But what about those undergraduates who will ultimately do graduate work in the social or clinical fields? The course outlined above will certainly be no less appropriate than electives in, say, economics or botany. And it will at least enable such students to analyse the data in their graduate theses, instead of finding out too late that the data are not analysable. As for those who do not intend to take graduate training, whatever psychology they require can best be provided by a suitable general course.

It is appropriate to end with a quotation from writings in another biological science by Tattersfield (27). "Twenty-five or more years ago, when I entered the field of research . . . the very whisper of the need for statistical analysis, falling upon the ears of the biological expert, was enough to bring down a storm of denials upon one's head. Although there may be some small residue of such a reaction still in existence, it now only persists in obscure nooks and crannies of the world of biological research."

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BOOK REVIEW

The Human Senses. By FRANK A. GELDARD. Toronto: University of Toronto Press (New York: John Wiley and Sons, Inc.), 1953. Pp. x, 365. $5.00.

Colour in Theory and Practice. By H. D. MURRAY (ed.). Toronto: British Book Service Limited (London, England: Chapman and Hall), 1952. Pp. xiii, 360. $14.00.

THERE are fashions in psychology just as there are fashions in clothes. There was a time in the history of psychology when much emphasis was laid on the topic of sensation. Today preoccupation with motivation, learning, attitudes, perception, thinking—all of them vastly important—has tended to crowd sensation into the background. For this reason, if for no other, it is of some significance that a book like Geldard's has appeared. Geldard holds that even though psychology be defined as a science of behaviour, we cannot ignore sensations. "All behavior is triggered by stimuli, and stimuli must have sense organs on which to operate." Geldard would probably agree that there can be no science at all without sensation. He certainly holds that there is need today for "sensory generalists" in psychology and that students beginning psychology need literally to be "brought to their senses."

The book itself covers the whole field of sensation and covers it better, I think, than any other book with which I am acquainted. After dealing with the origins of experimental psychology and very briefly with certain ways in which our knowledge of sensory processes has been applied in the field of human engineering, the writer considers each of the various sense departments. In every case he deals with the nature of the physical stimuli, the relevant anatomical and physiological conditions, and the psychological data. Physiological theories of vision, of audition, of temperature sensations, of smell, of taste, are presented clearly and impartially. The literary style of the book is unusually fine. It seems to this reviewer to be an excellent text for courses in sensation if and when those courses are offered in our psychological curriculum. As a supplement to experimental manuals it should prove of very great value in laboratory courses on sensory responses.

The book edited by H. D. Murray is a revision of an earlier volume published in 1939. Treatment of the objective aspects of colour stimuli is followed by twelve up-to-date and valuable chapters on the physiology and psychology of colour sensation, measurement, and matching, profusely illustrated with colour and half-tone plates. The book might properly be used as a supplement to Geldard's volume.

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